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Morten Jerven, and What Economists Do and Don't Get Wrong

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In a short space of time, Morten Jerven's work has influenced debates about economic growth and the nature of the underlying data, especially in the case of sub-Saharan Africa. Jerven (2013) is essential reading for anyone working in this area. His new book is also lively, readable, and thought-provoking. He argues that economists studying growth have misrepresented the African experience as one of long-run economic failure. In commenting on his summary of the arguments, I will also respond to the more detailed discussion and analyses in Jerven (2015). There is a lot to be said for many of the overarching points he makes, including the need for more attention to measurement; the importance of detailed country studies that complement theoretical models and statistical methods; and the value of seeing development as a process unfolding within particular contexts, which should not be analyzed just by taking models off the shelf.

But, some reservations. Jerven is sometimes too keen to take the weaknesses of one literature – more precisely, a subset of a subset - as representative of mainstream economic thinking. The views of economists about these issues are not based solely on cross-country regressions. In practice, many give that form of evidence little weight, at least unless other evidence points the same way. The claim that we are misled by mainstream economics, as Jerven occasionally suggests, would need more careful development than it receives here.

In his summary, Jerven indicates that he does not want the book to be misread. There is a risk of this, because some of the arguments lean on binary oppositions, without much interest in intermediate possibilities. There is a tendency to write as if the data are either accurate or uninformative; as if empirical research is either definitive or without value; as if economists are right or wrong; as if some lines of research can never lead anywhere; as if attempted generalizations must never have exceptions; as if we can know for sure which hypotheses are true or false. Jerven's own work makes clear that some of the relevant truths are inaccessible, and an understanding of growth will have to draw on open-minded detective work. Viewed as a larger project, the task requires that simple oppositions are treated with caution. I find myself agreeing with some of his conclusions, but not the route he has taken to get there.

There are other dangers in using growth regressions to develop a wider critique. The risk, not entirely avoided, is that empirical findings, and perhaps even whole literatures, have to be discounted to keep the broader argument moving. A casual reader of his book might conclude that empirical research in this area is homogeneous in its aims, in its conclusions, and in the naivety of its methods. This view relies on taking a small number of studies (including some of my own) as representative of a larger and richer whole, which has continued to develop and evolve. Over the last fifteen years, an informative literature using subnational data has emerged; Breinlich et al. (2014) survey some of this work and the relevant methods. The scope

for useful research of this kind, and even for learning from aggregate data using panel data methods, should increase as longer spans of data become available.

This may seem overly complacent, so I will try to respond to Jerven's critique in more specific and detailed terms. Jerven sees economists as trapped in a worldview in which Africa's lack of growth is taken as a given, and conflicting observations – such as past episodes of growth – are filtered out. But it's worth noting that economists have often been more optimistic than non-economists about the growth prospects for low-income countries, including those in sub-Saharan Africa. Growth economists often draw attention to the sustained success of Botswana and Mauritius; I have never heard it suggested that African countries are incapable of growth.

A more typical claim is that countries in sub-Saharan Africa face significant disadvantages in developing their economies and diversifying their exports: their distance from the largest global centres of economic activity; natural resource endowments which have shaped patterns of specialization; in some cases, large and sparsely-populated territories, which make it hard for the state to maintain order; a relatively brief history of political centralization; various legacies of colonialism, including imposed borders which created "artificial states", and low levels of schooling at the end of the colonial era; and a political context that has often allowed individual leaders to seize and maintain control of the state while doing little to improve governance or benefit the population as a whole. These structural disadvantages could help to explain the "Africa dummy" that was often found to be negative and statistically significant in the cross-section regressions of the early 1990s, but that may be the least important thing about them.

Jerven argues that pessimism is misplaced, because there are many episodes of growth in the historical record. I think he is right that those episodes are under-researched. As he says, examining them could lead to an understanding of institutions and politics that goes beyond simple-minded comparisons with developed countries. There are some interesting connections between his views and Albert Hirschman's emphasis on "possibilism": the idea that those who want change should not get too caught up in the outcomes that are probable, but look for possible ways through, including those that can circumvent obstacles. This view and its implications were set out in Hirschman (1971); see also Adelman (2013, pp. 450-454) and Lepenies (2008). When Hirschman discusses the subtle interactions of economic and political forces, the right of individuals and nations to a 'non-projected future', and what he calls the 'inventiveness' of the path of history, it is easy to see parallels with Jerven's criticisms of economists.

But taking a long view, the fortunes of East Asia and sub-Saharan Africa have diverged over a number of decades, from initial levels of development that were not dissimilar. In explaining Africa's stop-start growth, Jerven often emphasizes world market conditions, and especially movements in commodity prices. But in the medium run, the degree of exposure to world commodity prices is an endogenous outcome. It arises only for economies that have not successfully developed other lines of activity on a large scale, and thereby diversified their exports. In the sketch given here, Africa's structural disadvantages help to explain why growth has not been sustained, and why some countries remain exposed to external shocks.

These ideas have emerged from a variety of approaches, including theoretical models and empirical work. For example, Redding and Venables (2004) show that relative development levels are correlated with several measures of proximity to international markets. Their

empirical models are examples of the “levels regressions” that Jerven seeks to dismiss as irrelevant for policy, or a blind alley. But the findings hint at the potential importance of well-governed ports, and internal transport infrastructure, as a means of reducing the effective distance to external markets. Policy-relevant ideas emerge from research literatures in complex ways, and I am uncomfortable with treating particular research methods or lines of research as inherently unproductive.

Jerven is also wary of statistical work on the effects of governance, regarding survey-based measures of corruption or governance as too noisy to be informative. But the country-level measures are often based on averages, so the noise in individual responses will be averaged out. We have no reason to believe that the country-level outcome is meaningless, and several reasons to believe it is not. To take just one example, when Nunn (2007) finds that countries with better contract enforcement export relatively more in relationship-intensive sectors, it seems unlikely that mismeasurement of the institutions variables can explain his results.

A blanket objection to statistical work on growth is that the data are too poor to be informative; as Jerven puts it, “Garbage In, Garbage Out” (henceforth GIGO). In contrast, most economists tend to think “Garbage In, Nothing Out” (GINO). The GINO argument is that classical measurement error weakens correlations and partial correlations, rather than generating spurious results. Regression coefficients are biased towards zero, and statistical power reduced. In the case of two-stage least squares estimates, measurement error will weaken the explanatory power of instruments in the first stage. Overall, it will become harder, rather than easier, to reject the hypothesis that an effect is zero.

This argument can be taken too far, since if multiple variables in a regression are measured with error, coefficients can be biased away from zero. And there may be few good reasons to expect measurement error to be classical. But the GIGO argument seems risky, too. I’m uneasy with responding to statistical results by assuming whatever patterns of classical or non-classical measurement error are needed to explain them away: this can feel like a conclusion in search of an argument. What matters most is whether statistical findings are consistent with other approaches and forms of analysis, including the predictions of theoretical models, and work in other disciplines.

Nevertheless, I do agree with Jerven that growth economists have not properly acknowledged the problems raised by weak data. They have relied on instrumental variable methods as a solution, and have rarely examined the sensitivity of their results to varying degrees of classical measurement error. In linear models, this sensitivity can be examined using method-of-moments corrections (see Temple 1998). These require reasonable guesses of the extent of measurement error, and drawing on a range of estimates is one way to serve both readers who are deeply sceptical about the data, and those who are more optimistic.

This brings me to a further set of arguments: the case that Jerven makes that widely-used output data, in purchasing power parity terms, lack much genuine information about GDP per capita differences across countries in sub-Saharan Africa. One of his arguments, in both Jerven (2013) and Jerven (2015), relies on changes in country rankings across alternative sources of the numbers. I’m uneasy with this approach, since correlations and regression coefficients depend on sample moments, not rankings. If some countries have similar levels of GDP per capita, then adding modest amounts of noise can change rankings quite

substantially, even when that degree of noise would not have serious consequences for a set of regression results. So I regard this particular element of Jerven's case as unproven.

Another perspective is to contrast the variation in GDP per capita within countries over time, with the variation across countries at a point in time. The latter is typically much greater than the former. This suggests that replacing the dependent variable in levels regressions with an average over a number of years, tending to average out measurement errors and temporary fluctuations, would not change the overall results greatly. If this conjecture is confirmed in practice - and I do know enough about the cross-country literature that I don't take this for granted - it would then be hard to attribute the results to the idiosyncrasies of particular observations.

Some of Jerven's other objections relate to empirical models of growth that include a role for initial characteristics. These models risk implying that, for each initial characteristic, countries can be unequivocally ranked on a scale which plays the same role within all possible growth processes. But if we think of the many dimensions of societies and economies, even just those that can be measured, it would be surprising if none of them had a role in subsequent economic growth, even once we allow for different contexts and types of growth. Arriving at reliable answers on which matter, and how much, has proved difficult, but that does not mean that the entire empirical project is fundamentally misconceived or fatally Eurocentric. In practice, the most relevant concern is that the number of countries is too small for reliable inference.

Another possible objection is that empirical models with a role for initial characteristics are driven by preconceptions; economists build in assumptions about which countries can grow. The problem with this argument is that the models allow a wider range of possible outcomes. In the standard interpretation, growth regressions ultimately provide an account of relative steady-state development levels. When countries are below their steady-state paths, they will grow despite structural disadvantages, and there may also be growth along the steady-state path. There are grounds on which to dispute this account, but it's not one that rules out growth.

I think a better criticism is that these statistical models necessarily give a rather thin account of relative development levels, which would be modified and enriched - or sometimes overturned - by a detailed historical analysis. Here I agree with much of what Jerven has to say towards the end of his summary. In an ideal world, there would be more studies which achieved historical, political and economic sophistication at the same time, but this is not easily done. It is conventional to invoke the need for more interdisciplinary collaboration but, if good interdisciplinary work was easy, there would already be more of it. One answer might be to find new ways to advance debate in productive directions. The collection edited by Bayly, Rao, Szreter and Woolcock (2011) is subtitled "a necessary dialogue" and combines papers by historians with responses from economists and political scientists. Exchanges of this kind seem valuable, and there could be a case for some journal editors to innovate, and publish certain articles together with commentaries that deliberately cut across disciplinary boundaries.

The tenor of my comments has been critical, so I should reiterate that there are substantial areas of agreement. Some of Jerven's most important arguments, in this and his earlier work, could be summarized as "Don't trust the data, and don't trust those who do trust the data". I

understand the force of this advice partly because my own past work is not exempt from criticism. Jerven's distinctive combination of detective work and awareness-raising has been valuable, and will influence how future research is conceived and carried out. Some of the details of the arguments can be queried, and his accounts of the literature can be one-sided. But, in a remark sometimes attributed to Keynes, it is better to be roughly right than precisely wrong.

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